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LINKAGE VARIATION AND CHROMOSOME MAPS

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A recent paper in this journal by Detlefsen[1] is introduced as follows:

"There is a well intrenched concept of recent genetics that hereditary factors or genes may be given fairly definite loci on chromosome maps and that these maps correspond to or represent, roughly perhaps, the actual conditions in the chromosome. The basis for this attractive and suggestive view is the premise that the distance between two genes is necessarily proportional to the percentage of crossing over which these two genes show, other things being equal. If the distance which gives one per cent of crossovers is used as an arbitrary unit of measurement, then it follows that distances on the chromosome may be calculated in terms of this unit. It has seemed to me for some time that the antecedent in this hypothetical proposition contains a more or less gratuitous assumption. We do not know that the distance which gives 1% (or n%) of crossovers is a fixed unit. Stated differently, we do not know how constant the percentage of crossing over may be between two genes to which we give a fixed distance, i.e., our arbitrary unit of measurement may itself prove to be a variable. It may be possible for the distance which gives 1% of crossing over to differ in different females of the same population, or differ between stocks. In order to throw some light on these questions I began a set of experiments in 1916....."

Detlefsen then gives an account of an experiment in which crossover values for the white and miniature loci (in the X-chromosome of Drosophila melanogaster) ranging from 0% to about 33% have been obtained. Crosses between "high" and "low" lines are taken to indicate that a number of genetic factors influence the percentage of crossing over. The paper closes as follows:

"In view of these considerations it would perhaps be simpler to conclude that linkage is not a function of distance, i.e., crossing over is not necessarily proportional to distance. The distance between two genes may remain fairly constant, but the amount of crossing over depends upon numerous hereditary factors."

One unfamiliar with the literature of the subject would probably infer from Detlefsen's paper that the possibility of inherited linkage variations had not been taken into account by those concerned in constructing chromosome maps. In point of fact, the matter has not only been taken into account, but has been often discussed in the literature, as the following references will show.

In the first paper (1913) in which an attempt was made to construct a chromosome map,[2] the statement occurs (p. 49):

"Of course there is no knowing whether or not these distances as drawn represent the actual relative spatial distances apart of the factors.... we have no means of knowing that the chromosomes are of uniform strength, and if there are strong or weak places, then that will prevent our diagram from representing actual relative distances..."

Later than this[3] definite evidence for the existence of genes modifying the amount of crossing over was reported, and in the "Mechanism of Mendelian Heredity"[4] the situation was discussed as follows (pp. 67–68):

"It is not supposed, however, that the per cent of crossing over represents precisely the distance between the factors, for it may be that crossing over is more likely to take place in one region than in another. In that case the distances between factors in this region calculated from the amount of crossing over between them, would be relatively greater than the actual distance.... Sturtevant has found definite factors which alter the amount of crossing over in the chromosomes, and these factors actually do affect the amount of crossing over differently in the different regions.....however, ...the *order* of the factors remains unchanged."

The three papers mentioned above were all published before Detlefsen says he began his experiments, as was also Bridges,[5] demonstration that frequency of crossing over in the second chromosome changes with the age of the female. Since that time the question has been discussed again and again, and several cases of inherited linkage variations have been thoroughly analyzed.[6] The conclusions to be drawn from such evidence have been well stated by Morgan,[7] in a chapter devoted entirely to variationsin linkage (p. 125):

"It is to be understood, then, that when we substitute the idea of distance for crossing over values the term is not used in an absolute sense, but in a relative sense, and that it depends always on the conditions of the experiment. That the genes do stand at definite levels in the chromosome, and that in this sense they are definitely spaced, seems reasonable in the light of all the evidence bearing on this point; but even if they are so spaced that crossing over is a function of their distance from each other in the series, any influence that determines how often interchange between homologous pairs will take place would give the appearance that the actual distances themselves have changed."

As will be evident from the quotations given, the chromosome maps are intended to show the actual sequence of the loci, and the relative amounts of crossing over between them. The intervals between adjacent loci are not to be taken as necessarily proportional to the actual spatial distances between them—though that distance is evidently one of the elements concerned. When the amount of crossing over changes from any cause it is evident that we are dealing with a new system, and the intervals of the "normal" map will no longer be applicable (though in all cases so far investigated the *sequence* of loci shown in the "normal" map is unchanged). The "normal" map itself is based on the average results obtained under conditions in which no recognized disturbing factors (genetic or environmental) were known to be present. This map represents the results to be

expected from untried crosses, and has shown itself to be invaluable in everyday work. Detlefsen's own account is in agreement with this, for he says that his stock of white miniature has been used in class work and has always given the value to be expected from the published maps. When values that do not agree with the "normal" map are found, analysis has always shown some disturbing factor to be present; and in all cases where the point has been investigated it has been found that maps based on data uniform with respect to this disturbing factor are entirely self-consistent.

In view of these considerations it is clear that Detlefsen has misunderstood the significance of the published maps. The conception that he has attacked is one that has not been held or urged by those who have constructed chromosome maps.

[1] Detlefsen, J. A., These PROCEEDINGS, 6, 1920 (663–670).

[2] Sturtevant, A H., *J. Exper. Zoöl.*, 14, 1913 (43–59).

[3] Sturtevant, A. H., *Zs. ind. Abst. Vererb. Lehre*, 13, 1015 (234–287).

[4] Morgan, T. H., A. H. Sturtevant, H. J. Muller, and C. B. Bridges, New York, 1915, 262 pp.

[5] Bridges, C. B., *J. Exper. Zoöl.*, 19, 1915 (1–21).

[6] Sturtevant, A. H., These PROCEEDINGS, 3, 1917 (555–558), and *Carnegie Inst. Washington Publ.*, No. 278, 1919 (305–341); Muller, H. J., These PROCEEDINGS, 3, 1917 (619–626), and *Genetics*, 3, 1918 (422–499); etc.

[7] Morgan, T. H., *The physical basis of heredity*, Philadelphia, 1919, 305 pp.

ORIGIN AND HISTORY OF THE BEAR FAMILY IN THE WESTERN HEMISPHERE, WITH PARTICULAR REFERENCE TO THE RELATION OF THIS QUESTION TO PROBLEMS OF GEOGRAPHICAL HISTORY

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Although bears of many varieties are widely distributed in America and have been present in large numbers for a very long period measured in terms of years, the history of this group shows that as we go back in the geological record no evidence of representatives of the bear type are present in formations of the third geological period preceding the present in America, while they are known in considerable numbers in the rocks of this age found in the Old World. There is, therefore, good reason to believe that the bear group is derived from the other side of the earth and that the ancestors of the present American bears migrated to this continent at a time geologically not far removed from the present.

The bears of the world may be divided into two large groups, one some-

times known as the true bears, including the brown and grizzly types now widely distributed over the whole northern hemisphere. The second group is represented by the spectacle bear and is limited to the Andean region of South America.

In the geological period known as the Pleistocene, immediately preceding the present, true bears like the present black bear type were associated in North America with a group of large bears known as Arctotheres, closely related to the present spectacle bear of South America. In the same period South America was inhabited by numerous Arctotheres but contained no representatives of the true bears.

In the rocks of the Pliocene, or second geological period preceding the present, until recently no representatives of the bear group have been known in either North or South America. In the Pliocene deposits of Europe and Asia there are, however, remains of creatures closely related to the true bears and with these a second group originally known as the hyena bears, or the Hyaenarctos type, closely related in many characters to the Arctotheres, and through them related to the modern South American spectacle bears.

Assuming that the American bears are descendants of the Old World types, it is difficult to escape the conclusion that wide land connections existed between North America and Asia, and between North and South America, at such times as to permit the migration suggested by the present distribution. As the Arctotheres are represented in the Pleistocene of South America without accompanying true bears, it is logical to assume that the Arctothere group was the first to migrate to America and that it passed through North America reaching the South American region before the true bears had spread over North America. Investigations within the last few years have shown that in the Pleistocene deposits of North America both Arctotheres and true bears are present together down to the earliest strata in which remains of bears have been discovered. There has, therefore, been reason to assume that remains of Arctothere-like bears would be found in the Pliocene of America without associated remains of the true bear type. The point of the present paper is to call attention to the fact that recent carefully conducted investigations in the Pliocence deposits in several parts of North America have brought to light remains of bear-like forms which are intermediate between the typical hyena bears of the old world Pliocene and the Arctotheres of the American Pleistocene. Such a form is represented by a specimen found in the Pliocene of Oregon. This animal corresponds very closely to the most specialized of the hyena bears of India and also approaches the Arctotheres in its structure.

The evidence now available indicates that creatures of the hyena bear type came by way of broad land connections from Asia to North America

in Pliocene time; that these creatures represent the type of hyena bear most nearly approaching the Arctotheres and were widely distributed in North America. There is reason to believe that from this group the Arctotheres may have developed within the American region, and that the Arctotheres by way of a wide land bridge came to people South America.

The present spectacle bears of South America seem then to represent the last remnant of a group which originated in the Old World, was once widely distributed over the world, and included the largest of all known bears.

AN UNIDENTIFIED BASE AMONG THE HYDROLYTIC PRODUCTS OF GELATIN

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Of the known amino acids yielded by acid hydrolysis of proteins the work of various authors[1] has indicated that four, viz., histidine, arginine, lysine, and cystine, are distinguished from the others by the relative insolubility of their phosphotungstates in acid solution. On the basis of this fact Van Slyke[1] devised a method for separating these four amino acids as phosphotungstates, and determining them by utilizing certain characteristics of their chemical structure. The non-amino nitrogen of this group of amino acids is entirely in the histidine and arginine. The arginine was determined directly, and the histidine was estimated on the assumption that all the remaining non-amino nitrogen was in histidine.

This assumption we have tested by comparing the histidine content of a number of proteins as determined in the above manner with the values determined by Koessler and Hanke's direct colorimetric method.[2] In casein, edestin, and fibrin, the results by the two methods agree. But in gelatin the calculation based on the non-amino nitrogen indicates 6.1 per cent of the total protein nitrogen in the form of histidine, while the colorimetric method shows only 1.8 per cent. There is evidently among the products of gelatin hydrolyzed by hydrochloric acid a substance, or substances, hitherto unrecognized, precipitated with phosphotungstic acid under the conditions ordinarily utilized to precipitate the hexone bases.

In attempting to isolate the substance we have precipitated it by means of phosphotungstic acid with the other bases, have redissolved the precipitate and freed it from phosphotungstic acid. The histidine and arginine were removed by precipitation with silver sulfate and barium hy-

droxide, and the lysine as picrate. The residual solution contained an amount of non-amino nitrogen corresponding approximately to that determined by Van Slyke's method in excess of the arginine and histidine non-amino nitrogen.

Attempts to crystallize the free base or its derivatives have been successful only with the phosphotungstate. Recrystallization of the phosphotungstate yields a product in which the ratio, *Total N : Amino N* = 2 : 1. The free base prepd. from the recrystallized phosphotungstate is hygroscopic, and decomposes slowly when dried at 100°. It does not appear to be a peptide, for the ratio of amino to total nitrogen is not increased by boiling 40 hours with 20 per cent hydrochloric acid, nor by heating 8 hours in a bomb tube at 125° with 25 per cent sulfuric acid. We are engaged in the preparation of larger amounts of the substance in the hope of determining its structure.

[1] Literature quoted by D. D. Van Slyke, *J. Biol. Chem.*, 10, 1911 (15).

[2] Koessler, K. K., and Hanke, M. T., *J. Biol. Chem.*, 39, 1919 (497).

GENETICAL AND CYTOLOGICAL PROOF OF NON-DISJUNCTION OF THE FOURTH CHROMOSOME OF DROSOPHILA MELANOGASTER[1]

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A mutant type of *D. melanogaster* known as "Diminished" gave genetical results which proved the involvement of a "deficiency,"[2] i.e., a multilocal loss of genes, in the chromosome corresponding to the "fourth linkage group"[3,4] (see section I). Further exceptions to normal inheritance showed that non-disjunction[5] of this chromosome had occurred giving rise to (Diminished) individuals lacking one member of the fourth-chromosome pair (see section II). The deficiency in this case consisted therefore in the loss of an entire chromosome. The haploid nature of Diminished was then proved cytologically: it was found that in the cells of Diminished individuals only one small round chromosome was present instead of a pair (section III). This finding demonstrates the correctness of the view that the carrier of the genes of the fourth linkage group is the small round chromosome. A positive direct proof is provided that a particular autosome is the carrier of the genes of particular non-sex-linked Mendelian characters.

I. The features that in the aggregate prove that a deficiency is involved are:

(1) A definite complex of mutant characters: Diminished differs somatically from the wild-type in many characters, among which the principal are: smaller size throughout; bristles relatively shorter and more slender; body-color paler; thorax-pattern darker; eyes larger and roughish, somewhat denuded of hairs; aristae reduced or absent; wings slightly spread, blunter, and less clear in texture.

(2) Low productivity.

(3) Frequent stirility.

(4) Dominance.

(5) Heavy mortality: Outcrosses of "heterozygous" Diminished to wild-type gave an extremely variable proportion of Diminished; the bulk of the cultures ranged from 20 to 50 per cent, with a mean value of 34.5 (1772 Dm: 3366 wild-type). Since the Diminished offspring were expected to be as numerous as the wild-type, the mortality of the Diminished is very high, viz. 47%.

(6) Delayed hatching: Diminished adults begin to emerge from the pupa cases four or more days later than wild-type sibs. The delay is apparently due to a slow metamorphosis.

(7) Lethal when "homozygous:" When "heterozygous" Diminished was inbred there were 139 Diminished to 100 wild-type (1079 : 775). The expectation for such inbreeding, on the basis that "homozygous" Diminished is lethal and that mortality of the "heterozygous" Diminished is the usual 47%, is 106 : 100, which is somewhat under the observed ratio. The mortality of Diminished is extremely variable, and a mortality of 31% would give the observed ratio. Further evidence in favor of the lethal view is the fact that continued inbreeding did not increase the percentage of Diminished; also no Diminished individual was found that gave all Diminished offspring upon outcrossing.

(8) Inheritance as member of the fourth chromosome:

(*a*) The Diminished mutant is not due to a change in the first, second, or third chromosomes: Reciprocal crosses gave like results, and outcrosses of Diminished male to wild female gave Diminished offspring among the sons, so the Diminished is known to be neither cytoplasmic nor sex-linked in inheritance. Male back-cross tests with such well-known second-chromosome mutants as Star and black, and third-chromosome mutants, such as Dichaete, Hairless, pink and ebony, showed free assortment of these mutants with Diminished. Since there are only four chromosome pairs in *D. melanogaster* the above evidence proves that Diminished is caused by a change in the remaining or "fourth" chromosome.

(*b*) That Diminished is due to a fourth-chromosome change might be proved by means of male back-cross tests with the known fourth-chromosome recessive mutants bent and eyeless. But when crosses of Diminished to bent and to eyeless were made in order to secure the necessary

heterozygotes, it was found that all the F_1 flies that were Diminished were at the same time bent or eyeless, respectively. This fact made the intended back-cross tests both impossible and unnecessary. Crosses of bent, or of eyeless, to wild and to other mutants give only not-bent or not-eyeless progeny in F_1, while, on the other hand, crosses of Diminished to a great variety of recessives in the first, second, and third chromosomes have never given F_1 Diminished that showed any of those recessives. The fact that the Diminished allows "pseudo-dominance" of two dissimilar non-allelomorphic fourth-chromosome recessives proves that Diminished is due to a fourth-chromosome change, and one of a definite known class—that of "deficiencies."

(9) Pseudo-dominance of recessives in the affected section: Pseudo-dominance, such as found for the recessives bent and eyeless with Diminished, had already been met with in such cases as forked with Bar-deficiency,[1] vermilion with vermilion-deficiency,[6] and facet, abnormal and the white allelomorphs with Notch-deficiencies.[7] The explanation in these cases is that the section of chromosome from forked to bar inclusive, the section containing the vermilion locus, and the section from white to facet and abnormal inclusive, had been lost, or transformed in such a way as to have lost the normal action of its genetical materials. All previously enumerated features of Diminished are explainable by deficiency for a section of the fourth chromosome between and inclusive of the loci for bent and eyeless. A diminished individual is virtually haploid for this section.

The extent of a deficient region is measured in two ways: by observing to how many neighboring mapped loci pseudo-dominance extends, and by observing the amount by which the map-distance between adjacent not-included loci is decreased in the presence of the deficiency, there being no crossing over within the deficient region. The Diminished deficiency included the only two known fourth-chromosome mutants and thus made the second method impossible in this case.

(10) Exaggeration of mutants of affected section: The characters bent and eyeless in flies that are at the same time Diminished are more exrteme departures from the wild-type than are the simple bent and eyeless mutants. For example, the legs of the Diminished-bent compounds were much shorter and all the tarsi were like those of the hind legs of the most extreme normal bents; i.e., they were fused into hairy lumps instead of being chains of five slender joints. The eyes of the Diminished-eyeless compounds that hatched had about the same range as normal eyeless has; but among the dead pupae were found individuals in which the head was reduced to a mere proboscis on the end of the neck.

(11) Exaggeration of mortality of mutants of affected section: Crosses of Diminished to eyeless produced only 35 Diminished-eyeless (all males) to 2726 wild-type sibs, where equality is expected. The mortality of eye-

less in presence of Diminished is thus 98.7%. The mortality of simple eyeless is usually not greater than 5%, and the mortality of the simple Diminished is, as seen, about 47%. The mortality of Diminished-bent was even higher, viz. 99.5%, since only 9 Diminished-bents (males) hatched for 1727 wild-type sibs.

Since mortality changes must necessarily be only consequences of somatic changes, the exaggeration of the mortality of the Diminished compounds is another evidence of exaggeration of somatic changes—probably of the internal anatomy in addition to the observed superficial changes.

The mortality of Diminished, and of the Diminished compounds, is mainly due to somatic changes in the pupa stage: (*a*) If the mortality of Diminished individuals were due to changes in the larvae, then mass-cultures, because of the greater competition among the too numerous larvae, should show a lower proportion of Diminished than do sister pair cultures, but this was found not to be the case. (*b*) Examination of the culture bottles of the crosses of Diminished by eyeless showed that the number of Diminished-eyeless individuals that pupated but perished subsequently is approximately the same as the number of not-diminished flies that hatch.

(12) Exaggeration of certain mutants outside the affected region: Diminished exaggerates the third-chromosome dominant Hairless, and to a slight extent certain other mutants.

(13) Recurrent nature: Diminished has arisen independently at various times (see section II (1) below).

II. All the above features of Diminished, including exaggeration (10, 11, 12), are explainable by the theory of deficiency, and for nearly all there have been parallels in previously known cases of deficiency. Two features of Diminished appeared that made a sectional deficiency hypothesis inadequate.

(1) High frequency of re-occurrence: Diminished has been recorded some twenty-five times within the space of a year, which is too high a frequency to be explained without extreme difficulty on the sectional deficiency view. Notch-deficiencies have been recorded some 18 times, but this was in a period extending over eight years. Furthermore, there is evidence that the different Notches are not re-occurrences of the same deficiency, but show character differences and involve sections that are different in extent, though all possessing a common section in the neighborhood of facet. The different Diminished strains, on the oteher hand, are apparently identical. Most of the tests of this paper were carried out in parallel with strains No. 3, 4, 8, 15, and 3x.

(2) Exceptions to normal inheritance: In a cross of Diminished to bent there appeared a single Diminished male that was not bent. This exceptional male was tested and was found not to carry bent, although

his father had been homozygous bent. Furthermore, in a cross of Diminished to eyeless there appeared a single not-Diminished eyeless female. Genetical tests showed that this exceptional female was homozygous for eyeless, although her father had lacked eyeless entirely.

These exceptions to normal inheritance invited a comparison with the well-known exceptions to sex-linked inheritance which are due to non-disjunction of the sex chromosomes;[4] for it is evident that non-disjunction of the fourth chromosome would produce such exceptions, and also that it would give rise to the whole series of effects that are characteristic of deficiency. Diminished individuals are deficient not simply for a section of the fourth chromosome including the loci for bent and eyeless, but are deficient for a whole fourth chromosome; i.e., they are haploid for the fourth chromosome. A Diminished strain would arise from the occurrence of "primary" non-disjunction in either sex, whenever at maturation the pair of fourth chromosomes fails to disjoin and both members pass to the same pole, leaving the other pole without a fourth chromosome. Such nullo-IV eggs or sperm in union with normal haplo-IV sperm or eggs would give rise to individuals with only a single fourth chromosome. Judging by the number of independent strains of Diminished and by the two controlled exceptions, the frequency of primary non-disjunction of the fourth chromosome is of the same order as for the first.

If primary non-disjunction gave rise to nullo-IV gametes and thus to haplo-IV strains, the same process may also give rise conversely to diplo-IV gametes and thus to triplo-IV zygotes. No such individuals have as yet been indentified.[8] Aside from possible direct somatic effects of triploidy or tetraploidy for the IV chromosome, the presence of an extra chromosome would be detectable by duplication-phenomena,[9] just as the haplo-IV type was detectable by deficiency phenomena, and could be proved cytologically. The eyeless exception referred to above was produced by a diplo-IV egg, so that the process is well substantiated even here.

III. Cytological examination of the oögonial cells of Diminished individuals showed, as had been predicted, that only a single one of the small spherical chromosomes was present, while each of the other chromosomes was present in the usual paired condition. Four independent strains of Diminished, including the one that arose in the cross, were thus proved to be haploid for the fourth chromosome (fig.1).

IV. The haploid nature of the Diminished individuals made possible a back-cross determination of the bent eyeless crossover value without the necessity of a double recessive. The situation in the fourth chromosome is now comparable with that in the first, with haploidy for the fourth chromosome giving the Diminished type and pseudo-dominance of fourth-chromosome recessives, while haploidy for the first gives the male type and pseudo-dominance of sex-linked recessives. Females heterozygous for

bent and for eyeless were crossed to Diminished males, and the offspring furnished the equivalent of 17 crossovers in a total of 1987, or $0.86 \pm .14$ per cent of crossing over.

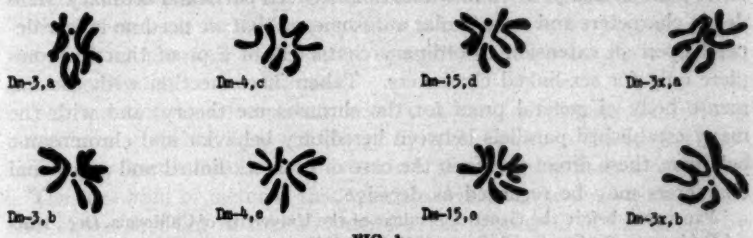

FIG. 1

An oögonial metaphase plate from each of eight "diminished" individuals, two from each of four strains, all figures showing a single round chromosome instead of the usual pair.

V. The cytological finding in the case of the haplo-fourths has put the theory of deficiency on a sound basis; for, each of the features met with in the cases described as deficiency has been met with in the case of the haplo-fourths. Thus, it has been shown, not simply that these characteristics might be produced by loss, but that in this specific case they were actually produced by loss.

VI. Both the character complex and the exaggeration effects of Diminished are due to a change in balance of the genes. Each character of the wild-type is the result of a balance between many genes that tend to modify the character in various directions. These "plus and minus modifiers" are apparently distributed at random in the chromosomes, so that if a particular short section of chromosome is removed, as in deficiencies, usually more plus than minus modifiers (or vice versa) of some character will be removed. In that case more minus than plus modifiers will remain in action, and a new character will appear, differing from the old in a minus direction.

VII. The genetic and cytological evidence in the case of Diminished has provided a positive identification of a particular chromosome—the small round one—as the carrier of the genes of particular Mendelian characters—bent and eyeless. Non-disjunction of the sex-chromosomes has previously furnished a similar identification of the rod-like chromosome as the carrier of the genes of certain sex-linked characters. Further work on non-disjunction of the X-chromosome has extended the identification to include so many sex-linked characters without exception that it may be accepted as general. Since the behavior of non-sex-linked characters had been shown to be the same as that of the sex-linked characters in a large range of particulars—in all points except those connected with the fact that the male is virtually haploid for this chromosome—the demonstration

that the sex-linked genes are carried by the X-chromosome left no plausible ground for believing that that proof did not hold for the ordinary characters and the ordinary chromosomes. However, the present case offers a direct and positive identification between particular ordinary Mendelian characters and a particular autosome, so that we need no longer depend upon an extension to ordinary characters of a proof that was complete only for sex-linked characters. Taken in connection with the immense body of general proof for the chromosome theory, and with the many established parallels between hereditary behavior and chromosome behavior, these direct proofs in the case of both sex-linked and autosomal characters may be regarded as decisive.

[1] Paper read before the Genetics Seminar of the University of California, Dec., 1920.

[2] Bridges, C. B., *Genetics*, 2, Sept., 1917 (445–465).

[3] Muller, H. J., *Jour. Exp. Zool.*, 17, Oct., 1914 (325–537).

[4] Hoge, M. A., *Am. Nat.*, 49, 1915 (47).

[5] Bridges, C. B., *Genetics*, 1, Jan.–Mar., 1916 (1–52, 107–163).

[6] Bridges, C. B., *J. Gen. Physiol.*, 1, July, 1919 (645–656).

[7] Mohr, O. L., *Genetics*, 4, May, 1919 (275–282).

[8] Dr. Little has recently reported such an identification (*Science*, 53, Feb. 18, 1921 (167)); but as I have pointed out in a note to *Science* (53, April 1, 1921 (308)) the facts as stated are more in conformity with the assumption of a "weak" allelomorph of eyeless, or of a linked dominant "minus" modifier of eyeless. (Since the above was written, triplo-fourth individuals have been identified among the offspring of triploid females (*Science*, 54, Sept. 16, 1921 (252)).

[9] Bridges, C. B., *Proc. Am. Soc. Zool.*, 1918.

NOTE ON THE ECONOMIC CONVERSION FACTORS OF ENERGY[1]

BY ALFRED J. LOTKA

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Communicated by R. Pearl, June 20, 1921

That there is some relation between physical energy and economic value has been more or less clearly recognized by several authors. So G. Helm, for example, conceives of an "economic energy," whose capacity factor is quantity (of commodity), and whose intensity factor is measured by the price paid per unit quantity. A certain justification for this standpoint may be found in the fact that on the one hand goods having economic value can be exchanged for, and in this sense converted into energy; while on the other hand the movement of commodities in an economic system, that is to say, the transfer of property by sale and purchase, is determined by differences in economic value in much the same way as the transfer of energy from one portion of a physical system to another is determined by differences in the intensity factor of the energy concerned in the change.

From Helm's[2] standpoint, money would represent the most fluid form of economic energy, convertible freely into any other form of economic energy. Attention to this rôle of money is also drawn by Ostwald, who, however, expressly makes the reservation that money, although bearing a certain resemblance to energy, is not essentially identical therewith.[3]

Nevertheless, as we all know, money is convertible, by purchase upon the market, into various forms of energy, at rates somewhat variable, yet in some manner determinate.

The question therefore arises, what is it that determines the ratio of conversion of various forms of energy by exchange upon the market?

The physicist is familiar with two kinds of conversion factors of energy.

The first kind of factor is that which enters into the analytical expression of the first law of thermodynamics, the expression of the constancy in ratio of the amounts of several forms of energy replacing each other in physical transformations.

The second kind of factor relates to that fraction of energy transformed, which can be recovered in a selected form, and expresses the "efficiency" of the transformation or of the transformer taken in view.

It is one of the central data of thermodynamics that the equivalence factor is always independent of the mechanism by which the transformation is effected, while the efficiency factor is thus independent in the ideal case of a reversible transformation

It is this independence which makes the two laws of thermodynamics so fertile as tools for drawing conclusions regarding the course of physical events. Anyone who has ever sought to show a perpetual motion inventor just why his machine cannot work will appreciate the economy of thought and language which is secured by direct application of these principles.

Now in the transformation of energy by economic exchange upon the market we are dealing with a third type of conversion factor. The physical relations here involved are so complex that we are apt to overlook altogether that they *are* physical. Still less do we ordinarily recognize their precise character.

A simple example may help to clarify the view: the case of the automatic vending machine, the penny-in-the-slot chocolate dispenser, for instance.

The salient facts here are:

1. A definite amount of money brings in exchange a definite amount of commodity (and of energy).

2. The physical process is a typical case of "trigger action," in which the ratio of energy set free to energy applied is subject to no restricting general law whatever (e.g., a touch of the finger may set off tons of dynamite).

3. In contrast with the case of thermodynamic conversion factors, the

proportionality factor is here determined strictly by the particular mechanism employed.

Reflection shows that all transformations of money or of economic assets of any kind into energy by exchange upon the market are of this character. It is always a case of trigger action. Somewhere there is a store of available energy, which can be tapped with an expenditure of greater or less effort. The payment of the price sets in motion the requisite machinery for the release of that energy (or for its transfer of ownership, the release being delayed at the discretion of the buyer).

In view of the entire absence of any general law regulating the ratio of energy released to energy applied in such case of trigger action, we may ask the question, how does it come about that economic conversion factors, economic ratios-in-exchange of different forms of energy, display any regularity whatever?

We have been accustomed, in thermodynamics, to disregard mechanism. In dealing with energy conversions by trigger action we must make a complete change of attitude. Here everything depends on mechanism. If we find any degree of regularity in the conversion factors, this must be due to regularities in the mechanism, i.e., in the human organism, and its social aggregations; and we cannot hope to make any progress in our understanding of the physical principles involved except by taking due account of the mechanism.

In view of the extreme complexity of the systems in which economic phenomena have their course, it may appear hopeless to attempt analysis. But reflection shows that the complexity is in the details; the broad underlying features resolve themselves into comparatively simple lines.

Our vital interest in physical energy arises out of the following facts:

1. The body of a living organism is *not* in equilibrium with its surroundings. In other words, the living body (and also the dead body, for some time after death, as popularly conceived), in its natural environment, is a source of available energy.

2. Though not in equilibrium with its surroundings, the living body is in an approximately steady state; furthermore, it is one of the essential conditions for the continuance of life that this approximately steady state be maintained. Extreme departures therefrom result in permanent displacement from the steady state, and, ultimately, in the dissipation of the available energy presented in the substance of the organism.

3. In the course of events a certain amount of available energy, varying according to the nature of the organism, is unavoidably dissipated, per unit of time.

It follows that every organism, in order to maintain the steady state necessary for its survival, must be provided with devices for capturing available energy.

Furthermore, in the competition which takes place among organisms, the advantage must go to those whose energy-capturing devices are most effective in directing available energy into such channels as are favorable to the preservation of the species.

In man, as an organism of the animal type, the mechanism for capturing available energy comprises three elements:

1. Sense Organs, or Receptors, whose function is to establish a certain rather close correlation between the state of the environment and that of the individual. Their function, in fact, is to *depict* the external world in the organism, to apprise him of the state of his environment.

2. Organs of Operation, or Effectors, such as hands, feet, etc., by means of which the individual reacts physically upon his environment so as to modify it, or to modify his relation to it (as, for instance, by locomotion).

3. Organs and Faculties of Adjustment, or Adjustors, whereby the action of the Effectors is adjusted in accordance with the indications furnished by the sense organs (receptors), and with the needs of the organism.

Among these adjustor faculties one which figures prominently and plays an important rôle is what we may call the *sense of values*, that faculty which we exercise when we are confronted with two or more alternative courses of action, from which one must be selected. In such a situation we choose the course which appears to us, subjectively, the more *desirable*, and we do not ordinarily give any thought to the question as to what may be, objectively, the significance of "desirability," any more than we do ordinarily concern ourselves, in viewing a landscape, as to the particular wave-length, the objective characteristic, of the light which to our subjective judgment appears "green."

And yet, for the interest of the species, it is evidently far from indifferent what may be the objective characteristics of those things which to us appear, subjectively, desirable. Upon the proper adjustment of the sense of values, of the "tastes," to certain objective realities depends the welfare of the species. A nation of drunkards, for example, is not destined to figure among the winners in the struggle of evolution.

If, then, the physicist has been interested in discovering the objective significance of the indications of our sense organs, if he has thought it within his province to investigate the relation between color and wave-length, between musical pitch and frequency, then it is equally a problem for the physicist to enquire into the physical basis of economic value.

The ground has been prepared for this enquiry by the mathematical economists in their hedonistic calculus. A person having at his disposal a given sum of money, and being confronted with the necessity of choosing between different ways of spending it, will seek so to apportion his ex-

penditures as to procure for himself a maximum of pleasure. So, for example, if the choice lies between two commodities C_1 and C_2 (which may, in particular, be certain forms of energy) he will purchase quantities q_1 and q_2, respectively, such that, if his total pleasure derived from the purchase is Ω, then

$$\frac{\partial \Omega}{\partial q_1} dq_1 + \frac{\partial \Omega}{\partial q_2} dq_2 = 0 \qquad (1)$$

where dq_1, dq_2 are small, arbitrary increments in the quantities of the commodities C_1, C_2 purchased. If p_1, p_2 are the prices upon the open market, say, per unit quantity, of commodities C_1, C_2, respectively, then evidently the corresponding increment in expenditure is $p_1 dq_1 + p_2 dq_2$. But the sum of money to be spent is fixed. We must have, therefore,

$$p_1 dq_1 + p_2 dq_2 = 0 \qquad (2)$$

It follows, then, from (1) and (2), that the individual will so apportion his purchases, that

$$\frac{\partial \Omega}{\partial q_1} \Big/ \frac{\partial \Omega}{\partial q_2} = \frac{p_1}{p_2}. \qquad (3)$$

If the commodities C_1, C_2 are "necessities of life," whose influence upon the life of the species is to increase r, the proportional rate of increase of the species, then, evidently, the interests of the species demand that the purchases be so apportioned as to make r a maximum, and thus, by similar reasoning, to make

$$\frac{\partial r}{\partial q_1} \Big/ \frac{\partial r}{\partial q_2} = \frac{p_1}{p_2}. \qquad (4)$$

If we regard the sense of values as a device for attaining, as nearly as may be, the adjustment (4), then we see that a perfect sense of values would make

$$\frac{\partial \Omega}{\partial q_1} \Big/ \frac{\partial \Omega}{\partial q_2} = \frac{\partial r}{\partial q_1} \Big/ \frac{\partial r}{\partial q_2}. \qquad (5)$$

So that, in a community of organisms endowed with a sense of values thus perfectly adjusted, the marginal utilities $\frac{\partial \Omega}{\partial q}$ (and hence the prices on an open market) would be proportional to the partial derivatives $\frac{\partial r}{\partial q}$. In practise this proportionality will not be actually reached; but it represents the limiting state towards which a race must tend to evolve. For, a race having tastes radically at variance with the exigencies of race propagation (measured by the rate of increase r), could not long survive, as pointed out years ago by Herbert Spencer.[4]

It is this tendency to approximate (perhaps somewhat remotely) to the adjustment (5), which gives rise to fairly definite economic interconversion factors of different forms of energy, as purchased upon the market, and applied to specific uses. There is no need to invoke a specific "economic

energy;" on the contrary, our analysis, in which the quantities $\frac{\partial r}{\partial q}$ play a significant rôle, lends little support to Helm's view regarding the physical dimensions of economic value. Such value, or, to use more exact terms, "marginal utility," does not, in the analysis here presented, appear as the intensity factor of an energy.

[1] Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University, No. 40.

[2] Helm, G., *Die Lehre von der Energie*, Leipsic, 1887, pp. 72 et seq.

[3] Ostwald, W., *Energetische Grundlagen der Kulturwissenschaften*, Leipsic, 1909, p. 155. *Die Philosophie der Werte*, Leipsic, 1913, pp. 260, 314–317, 326, 328. Among other writers who touch on the subject of the relation of economic value and price to energy are: Budde, *Energie und Recht*, Leipsic, 1902, p. 56; Winiarski, "Essay sur la Mécanique Sociale," *Revue Philosophique*, 1900, vol. 49. p. 113. See also J. Davidson, *Qu. J. Economics*, Aug., 1919, p. 717.

[4] And for others whose pains and pleasures he makes his own.

THE BEHAVIOR OF HOMOLOGOUS CHROMOSOMES IN A TRIPLOID CANNA

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Communicated by C. B. Davenport, June 12, 1921

In a diploid flowering plant we may regard the chromosome group as consisting of a number of sets of chromosomes, two in each set. When, there are differences of size in the group, as has been shown in Yucca,[4] Crepis,[6] Morus,[5] Datura, and two dozen or more other species,[8] the two chromosomes of each set are of the same size; they form bivalents distinguishable by their sizes at the first maturation divisions of the pollen-mother-cells;[1] and they can consequently be seen to replace one another in the haploid groups of chromosomes of the microspores and megaspores. Evidence from breeding, especially in cases of non-disjunction, also shows conclusively that the chromosomes of one pair are nearly equivalent or homologous. We may then define a diploid plant, in the strict sense, as one having a chromosome group formed of pairs of homologous chromosomes. A triploid plant possibly comes either from the cross of a tetraploid and a diploid, or from the union of a normal germ-cell with a pollen-grain or egg-cell in which one of the cytoplasmic divisions of maturation has been omitted. (I have found one cytoplasmic division commonly omitted, especially after cold, in the pollen of Stizolobium, Datura, etc.) The differences in size in the chromosomes of a triploid mulberry,[7,8] and of a triploid Datura, show that the sets consist of three similar chromosomes each. The breeding results with plants in which one set consists of three chromosomes, while the other sets have only two, show

that the three are homologous.[2] Hence we may define a triploid plant, in the strict sense, as one having a chromosome group made up of a number of sets of three homologous chromosomes.

In diploid plants there is an attraction of some kind between the two homologous chromosomes which form a bivalent or dyad at the first division. We may expect an attraction also between the three homologous chromosomes in the corresponding triploid plants, leading to the

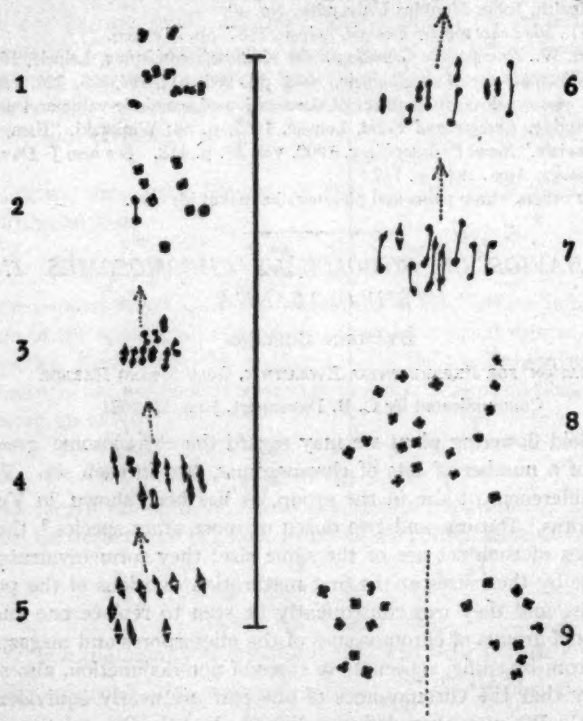

FIG 1

The drawings were made at stage height, with the Abbe camera. A Leitz 2 mm. apochromatic objective of 1.3 aperture, and compensating ocular 6 were used. An achromatic condenser of 0.9 aperture was focussed on the object so as to give critical illumination. The pollen-mother-cells were fixed and stained with iron-aceto-carmine (1). The assortment of the chromosomes in the pollen-mother-cells of a diploid Canna clone. This was obtained under the name "Madame Crozy." (1) Prophase of the first division, showing 9 bivalents. (2) Metaphase of the first division, showing 9 dyads, one already divided. (3), (4), (5), (6), and (7) are stages of early anaphase, showing division of the 9 dyads into 9 + 9 chromosomes. (The arrows indicate the situation of the polar axis.) (8) After the first division, showing 18 chromosomes. (9) The two groups in the metaphase of second division, separated by a cell-wall.

formation of triads. Thus Wilson[9] observed that three small chromosomes in Metapodius were grouped into a triad in the first division, and separated so that two went to one pole and one to the other. Osawa[5] found in triploid mulberries that some of the single chromosomes might be attached to the dyads in the first division of the pollen-mother-cells. In one triploid Canna (and in a triploid Datura), however, I found that all the chromosomes regularly form triads in the prophase and metaphase of the first division, and pass, two and one, in a random manner, to either pole.

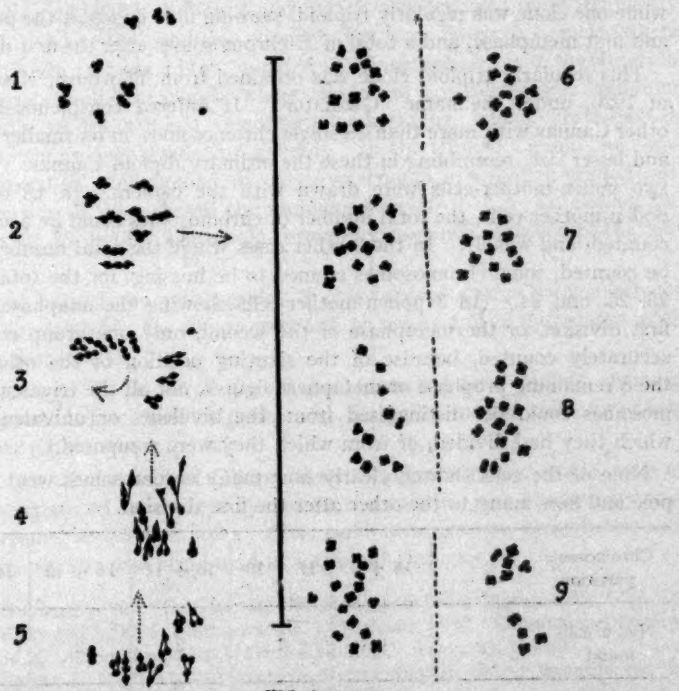

FIG. 2

The assortment of the chromosomes in the pollen-mother-cells of a triploid Canna. (1) Prophase showing triads. One chromosome separated from its corresponding dyad. (2), (3), (4), and (5) metaphase and early anaphase groups. In (3), one chromosome has again separated from its two partners. (6), (7), (8), and (9) metaphase plates for the second divisions, with cell-wall between, showing, respectively, 14 + 13, 15 + 12, 11 + 16, and 17 + 10 chromosomes.

In 1920 I grew 46 differently named clones and species of Canna, obtained, without particular selection, from three leading dealers of New York, Philadelphia, and Florida, and from the U. S. Department of Agriculture. The chromosomes of 31 of these were counted in the first or second divisions of the pollen-mother cells; about 250 groups being

drawn with the camera. Of these 31 clones, 22 showed nine dyads or bivalents, which split into 9 + 9 at the first division (fig. 1); 3 clones had a total of 18 single chromosomes (or 9 bivalents), which divided at the first division into 8 + 10, or other unequal numbers, not commonly into 9 + 9; 5 clones were probably completely or nearly triploid, and irregular in their first division, resembling in this the triploid mulberries of Osawa,[5] and like them showing a smaller number of chromosomes after the first division than the triple number (in these cases, 24 to 26 instead of 27); while one clone was regularly triploid, showing nine triads at the prophase and first metaphase, and a total of 27 chromosomes after the first division.

This regularly triploid clone was obtained from Thorburn, New York, in 1920, under the name "Gladiator." It differed conspicuously from other Cannas with more than 18 single chromosomes, in its smaller flowers and lesser size, resembling in these the ordinary diploid Cannas. Thirty-two pollen-mother-cells were drawn with the camera. In 18 of these pollen-mother-cells, the total number of chromosomes could be accurately counted, and was 27. In the 3 other cases where the total number could be counted, some chromosomes seemed to be missing, for the totals were 25, 25, and 24. (In 3 pollen-mother-cells showing the anaphase of the first division, or the metaphase of the second, only one group could be accurately counted, because of the slanting position of the other. In the 8 remaining prophase or metaphase figures, not all the trivalent chromosomes could be distinguised from the bivalents or univalents into which they had divided, or from which they were composed.)

Nine of the cells showed clearly how many chromosomes went to one pole and how many to the other after the first division.

Chromosome partition	18 + 9	17 + 10	16 + 11	15 + 12	14 + 13
Nos. of cells found	0	1	2	2	4
Calculated on random distribution	0.04	0.3	1.3	3.0	4.4

(In the triploid Datura the chromosomes have been accurately counted in 64 pollen-mother-cells after the first division, and the distribution corresponds with a random one.)

Three of the early anaphase plates showed how the triads divided. Taking for "up" the upper side of the camera drawing, which was a random position, we have for the position of the two's with regard to the equator (fig. 2, Nos. 4 and 5):

	Up	Down	Doubtful
Third pollen-mother-cell	4	4	1
Second pollen-mother-cell	4	3	2
First pollen-mother-cell	2	4	3
Totals	10	11	

In several prophases, one chromosome was disconnected from its corresponding dyad (fig. 2, Nos. 1 and 3). If this occurred more frequently it would lead to the condition found in the large-flowered Cannas, where triads, dyads, and monads are mingled at the metaphase; or to the situation of Osawa's mulberry clones, where triads are apparently less common; or finally to the triploid Oenothera lamarckiana, which, according to Geerts,[3] shows only dyads and monads. In the triploid Canna, one complete count of both second divisions was made in a pollen-mother-cell. On one side of the cell-wall the numbers were 15 + 1 + 12, and on the other side, 13 + 13. The single chromosome seemed attached to the cell-wall.

About half of the pollen-grains of this triploid Canna were nearly or quite empty; while the others were full or nearly full of cytoplasm, with one or more nuclei.

Summary.—(1) Most of the Cannas examined were diploid, showing nine dyads before the first division in the pollen-mother-cells, and these in most plants separated into 9 + 9.

(2) One of the triploid Cannas showed commonly nine triads, each of which separated into two and one on the spindle, in a random manner with regard to the two poles. (The same arrangement, though less easy to demonstrate, was also found in a triploid Datura.)

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THE DESTRUCTION OF PHOSPHORESCENT ZINC SULFIDES BY ULTRA VIOLET LIGHT

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It has long been known that the phosphorescence exhibited by certain alkaline earth sulfides is gradually destroyed when they are subjected to the action of alpha particles from radio-active matter.[1] It has also been found that a permanent destruction of the phosphorescent properties of such sulfides is caused by the bombardment of the sulfides with canal[2] rays or with cathode[3] and beta[1] rays. In this process of destruction, the sulfides are caused to phosphoresce brilliantly by the destroying agents, and the destruction is accompanied by a change in color of the sulfide, generally a darkening.

In 1919 J. Perrin[4] and more recently R. W. Wood[5] have shown that in the process of fluorescence, a process assumed to be somewhat similar to phosphorescence, the fluorescing molecules undergo chemical changes which result in the loss of the fluorescent power. In other words light of a given wave-length falling upon a molecule of fluorescent substance causes it to emit light of a different wave-length, and in this process the molecule is chemically changed so that it can no longer fluoresce.

The detection of the destruction of the fluorescent properties of certain substances is complicated by the fact that the fraction of molecules that are destroyed per second is relatively so minute that the time of exposure required to produce a measurable effect must be very long. As Perrin showed, this time may be much reduced by using very thin films of the solutions of fluorescing molecules. Wood has shown that it may also be much reduced by using intense sources of light. In view of the destruction of the phosphorescence of zinc sulfides by other agents which cause them to phosphoresce, and in view of the powerful technique developed by Perrin and Wood for the destruction of the fluorescent property by light, it occurred to one of the writers to attempt to detect a destruction of the phosphorescent property of the zinc sulfides through the action of light. The only reference to observations on such a destruction yielded by a survey of the literature consisted of the following quotation taken from a long paper by Baerwald[3] on the effect of canal rays on phosphorescence. Baerwald states "It is remarkable that intense ultra violet light from a mercury arc also destroys the phosphorescence of zinc sulfide. As it becomes dark in the process we may suspect a chemical change of the sulfide into an allotropic form,....."

For the preliminary experiments which are being reported here, three

different samples of phosphorescent zinc sulfides were used. One was a sample obtained from the Radium Standard Chemical Company in 1918, the others were two samples obtained in France from MM. Guntz and Ziegler of the powder works at Bouchet in 1918. The specimens to be studied were prepared by sifting the fine crystals upon small circular plane glass plates, 7 mm. in diameter which had been moistened with a very dilute solution of collodion in amyl acetate (about 3 drops of an ether collodion solution of the consistency of "New Skin" in 100 cc. of amyl acetate). Microscopic examination of the specimens showed that they were covered with a uniform layer of the sulfide occupying about 99% of the surface. The glass plate was fastened to a plate of thin transparent quartz, with the layer of sulfide pressed firmly against the quartz. The back side of the glass plate was then painted with optical black to permit the photometry of the sample. At the same time that the specimen was mounted on the quartz plate a control specimen as nearly the same as possible was similarly mounted on a glass microscope slide.

The quartz-mounted specimen was then placed in the image of a quartz Hareus mercury arc thrown upon the wall by a quartz lens of 3 cm. aperture and 15 cm. focal length. The arc operated continuously at about 2.5 amperes in a light-tight box. A blackened thermometer bulb placed in the image of the arc showed a rise in temperature of but 0.5° C. above that of the room, so that the heating of the specimen produced by the arc was not of importance. The glass-mounted control sample was kept in a light-tight box and was removed from this only at the time of photometric measurement.

The phosphorescent intensities of the quartz-mounted specimen and the control were compared by photometric measurements on the phosphorescence exhibited by them when studied in a simple phosphoroscope. The specimens were mounted on the window of a small photometer with the blackened side towards the window. This window was diffusely illuminated from behind by a small automobile headlight lamp. The intensity of illumination of the window was varied, and hence the photometry was accomplished, by changing the distance of the lamp from the window until the phosphorescent specimen faded against the window which served as its background. A color match between the lamp and the specimen was obtained by the use of absorbing screens of colored glass. In the photometry the specimens were excited to phosphorescence by the light from a 75-watt lamp focussed on them. The exciting light was put on and cut off in the usual way between the intervals of observation by means of slits in a metal disc rotated at a high speed, the procedure being in all respects the standard procedure used in phosphorescent measurements. After the quartz sample had been exposed to ultra violet light for a suitable interval of time, its intensity was compared with the

control as above. The results obtained on three samples are tabulated below.

FRENCH SAMPLE NO. 2

Intensity	Time in hours	Date	Comments
1.00	0	3-21-21	
0.74	94	3-27-21	
0.76	159	3-30-21	
0.67	221	4- 2-21	
0.66	315	4- 6-21	
0.57	428	4-11-21	
0.69	527	4-14-21	Exposed to Cl_2 contamination in air of room
0.51	665	4-20-21	
0.95	689	4-22-21	Exposed to Cl_2 contamination in air of room
0.71	834	4-28-21	
0.84	858	4-29-21	After 24 hrs. in Cl_2 gas in flask

FRENCH SAMPLE NO. 3

Intensity	Time in hours	Date	Comments
1.00	0	3-25-21	
1.00	26	3-26-21	
.43	64	3-28-21	
.41	101	3-30-21	
.34	163	4- 2-21	
.23	257	4- 6-21	
.24	370	4-11-21	
24	466	4-14-21	Slight Cl_2 contamination in air of room
.27	604	4-20-21	
.49	628	4-22-21	Exposed to strong Cl_2 in air of room
.30	771	4-28-21	
.26	033	5- 9-21	

AMERICAN SAMPLE

Intensity	Time in hours	Date	Comments
1.00	0	3-30-21	
.84	95	4- 2-21	
.59	304	4-11-21	
.60	399	4-14-21	Slight Cl_2 contamination in air of room
.82	589	4-22-21	Exposed to strong Cl_2 contamination in air of room
.80	728	4-28-21	
.57	818	5-2-21	
.53	986	5-9-21	

In the first column are given the intensities of the specimens compared to the controls corresponding to the number of hours exposure to the ultra violet light recorded in the second column. In the third column are given the dates on which the measurements were made; while the fourth column is reserved for comments. In every case it may be seen that there is a marked reduction in the phosphorescent intensity with

the increase in time of ultra violet illumination. In fact the French specimen No. 3 is seen to have dropped to a fourth of its initial intensity after 300 hours in the image of the arc. Though the changes are not very regular, the reduction in most cases is continuous with time, until the measurements made on the 14th of April are reached. The irregularities up to that date were doubtless due to the difficulties involved in the photometry of weak sources. On the 13th and 14th of April the air in the room in which the specimens were being exposed was mildly contaminated with chlorine gas used in the course of other experiments which were being performed in the same room. Again, from the 18th to the 22nd of April the air was contaminated by this gas, the contamination being specially strong on the 21st and 22nd. It is seen that the presence of the chlorine gas apparently arrested further decrease in intensity of two of the samples after April 14th; while it actually *increased* the intensity of the French Sample No. 2 which was subjected to a particulary heavy dose of the gas. On the 22nd, after the exposure to the strong chlorine, it is seen that all the samples showed marked increases in phosphorescent intensity. This was followed by a decrease in intensity on further illumination in the absence of chlorine after that date.

The reduction in intensity of the phosphorescence was in all cases accompanied by a darkening of the sulfide. Microscopic examination showed that the characteristic yellow color of the sulfides had disappeared and was replaced by a slight but uniform blackening throughout the crystal mass. This blackening was much like that of some glasses that have been subjected to cathode ray bombardment. To eliminate the possibility of this blackening having been caused by the collodion, a sample of the collodion film was exposed to the light for many hours. It showed no signs of blackening. A specimen of sulfide made up without the use of the collodion furthermore showed the same decrease of intensity and the same blackening on exposure to the ultra violet light as was shown by the samples using collodion. Following the accidental exposure to the chlorine gas it was observed that the specimens which had been blackened by exposure had regained their original color in a ring about 2 mm. wide at the edges of the specimens, i.e., where the chlorine had diffused inwards. It was these portions with restored color which showed the marked increase in intensity observed. The blackened centers of the specimens remained dull. Finally, to ascertain that it was the chlorine that caused the restoration of the color the French Sample No. 2 was placed in a flask with chlorine gas over night. On examination it was found to have regained its yellow color throughout its mass. On measurement, even though its edges were moist, due to the deliquescent nature of some of the reaction products, the intensity of the specimen was found to have increased markedly, as the table shows.

There is then little doubt in concluding from these experiments that the action of intense ultra violet light causes a destruction of the phosphorescent power of the zinc sulfides. This destruction is accompanied by chemical changes in the sulfides, resulting in a change of color, (possibly in a reduction or partial reduction of the polysulfides of the impurity (here probably Cu) required to produce the phosphorescent centers). It is further obvious that the exposure of the crystals to a powerful oxidizing gas such as chlorine at least in part restores both the original color and the ability to phosphoresce. It may accordingly be quite possible that the action of ultra violet light in this case is similar to that of ordinary light in the photographic processes, only much slower.

The results so far obtained hardly permit one to say much about the form of the curves of decay of phosphorescent intensity with the time of ultra violet illumination. They indicate, however, that the curves may either be the sum of a group of curves having the form $I = \frac{I_0}{At}(1 - e^{-At})$ given by Rutherford[6] for destruction of phosphorescence in the case of alpha particles where the intensity of the alpha particle radiation falls off exponentially with the distance below the surface; or they may be the sum of a group of exponential curves having different constants. Such curves would be expected in the case of measurements made on the decay of *total* phosphorescence with ultra violet illumination, for it is known that the phosphorescent light is composed of groups of different phosphorescent wave-lengths, each group probably having its own susceptibility to destruction, and hence each having its own time rate of destruction by ultra violet light.

The destruction of the phosphorescent power of these sulfides by ultra violet light noted above might point to a close analogy between the phenomena of phosphorescence and fluorescence in the light of the recent work of Perrin. Such an analogy would be in accord with the conclusion arrived at by Rutherford[6] in the case of the destruction of the phosphorescence by alpha particles, viz., that the light emitted under alpha particles bombardment *accompanies* the dissociation and *destruction* of the active centers. If this interpretation is correct, and if phosphorescence and fluorescence are closely related, the theory of Schmidt and Wiedemann,[7] Lenard,[7] and Merritt,[7] explaining the mechanism of phosphorescence will have to be modified. This theory ascribes phosphorescence to the return of ionized portions of the active centers (presumably electrons), to the centers from which they were removed by the exciting agent. Such a change does not involve a *permanent* destruction of the active center. According to Perrin the active center is permanently chemically changed in the process by which the substance emits its fluorescent radiation. It therefore follows that unless the production of phosphorescent light and

the destruction of the active centers by the ultra violet light are two independent phenomena the existing theory must be altered. Further experiments are needed to decide this point. The writer's thanks are due to Prof. R. A. Millikan for his kind interest and criticism.

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*ACOUSTIC PRESSURE DISTRIBUTIONS, CHIEFLY IN RESERVOIRS AND IN PIPES**

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Introductory.—In *Science* (May, 1921) I described an available method of pressure measurement in connection with acoustic phenomena, the method consisting essentially of a shallow mercury U-tube, the head of which is read off by displacement interferometry. Thus the fringe displacement s (occurring throughout the following paragraphs) will vary directly as the head in question and therefore with the pressure in the region under investigation. When the shank of the U-tube communicating with the sounding region is quite closed or quite open, significant results are either absent or without interest; but when the sound leak is reduced to the dimensions of a pinhole in foil, or the like small aperture, relatively large pressure increments or decrements are usually found to reside in the vibrating medium, varying with frequency and particularly marked when the region is in resonance with the sound generator. As an instrument for this purpose, a telephone, actuated by a small induction coil and a motor current interruptor is preferable; since non-symmetric inductions conduce to greater sharpness and strength in the phenomena to be observed. The current so obtained may be reversed, changing all the pressure increments into corresponding dilatations. The effect of the symmetrical induction of the magneto does not admit of commutation;

*From a forthcoming Report to the Carnegie Inst., Washington, D. C.

but it is nevertheless definite. When a large resistance, r, put in the telephone circuit produces a fringe deflection, s, the equation sr = constant is near enough for practical purposes and thus the passage from s to p (when called for) is very convenient.

It is the object of the present paper to continue these experiments by treating pressure distributions in vibrating regions of different size and in slender pipes. The case of wide organ pipes I have also investigated, but the results must be given later.

In figure 1, the two shanks of the mercury U-tube at least 6 cm. in diameter (R, air volume 48 cm.^{3}, R', air volume 135 cm.^{3}), are closed on top by glass plates, to receive the rays of the vertical interferometer; i.e., the component rays enter R and R', normally to the diagram.

U-tube Used Differentially.—The two different reservoirs R and R' of the U-tube, figure 1, were placed in communication by the branch pipe b, b', containing the three way cocks, D, E, shutting off the air vents a, d, and leading to the telephone T through the quill tubes t and t'. The pinhole is in the second branch t'' at O and a cock, C (here open), is interposed. I had anticipated a differential fringe deflection, owing to the different resonance volumes at R and R'. The result, however, was absolutely negative. Each reservoir (R, R') thus acts like an open air communication with reference to the other, so that the effect of the pinhole valve O vanishes. On using either side separately (t b R closed, R' D a open to the atmosphere, or t b' R' closed, R E d open) the normal behavior at once appeared.

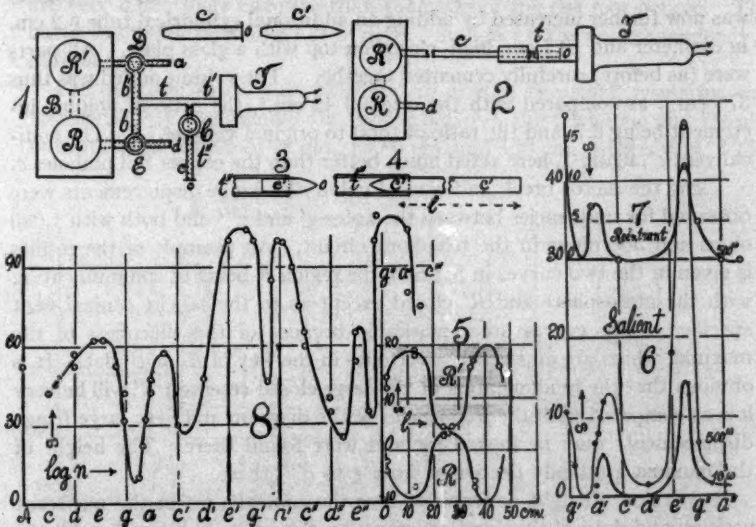
The only way of securing a differential effect detected, was by elongating either branch (b, for instance, by inserting a long piece, 40 to 80 cm., of rubber tubing). No doubt, this is merely equivalent to stopping, partially, access to either chamber, R or R'.

A variety of correlative experiments were made with the simple (non-differential) apparatus (t b R closed R' D a open). I have elsewhere referred to the absence of fringe displacement (nearly) when the copper foil carrying the pinhole is cemented to the mouth of a funnel tube; but the prolongation of the quill tube t'', if the diameter is not increased, is almost indefinitely permissible.

The experiments made suggest a method of obtaining an effect which is at least apparently differential. For this purpose the cock C (fig. 1) is to be closed, so that c is inactive, and the pinhole c (fig. 1) to be inserted either into the branch b' or b. With a normal fringe displacement of 40, the pinhole in b' gave a displacement of 38 fringes; in b correspondingly 27 fringes. Hence the large volume R' is more favorable to a larger displacement than the smaller volume R. It will be observed at once that here these reservoirs act merely like the outside air in the case of the original experiment. Thus there is no true differential effect resulting from the

size of resonators, whether the region be closed, open or partially open.

Conical Vents Reversible. Periodicity.—The conical vents c' (fig. 1) may be inserted into the branch tube t'' in two ways, either in the salient position a with the convex surface around the pinhole O outward (fig 3); or in the reentrant position (fig. 4) with the convex surface at O inward. The results obtained are usually reversals of each other, so that a pressure excess is liable to be on the concave side of the conical vent. These results were always consistent in character; but it was soon found that the strength of the telephone current and the length l of the tube $c'c'$ in the reentrant position (fig. 4) greatly modified them. When there is no resistance in circuit, i.e., when the telephone sounds harshly, reversal ceases, so that either the case 3 or 4 produces a pressure within; but even here, on closing the circuit, the fringes in case 4 are seen to move first toward a dilatation and then turn in the direction of pressure.



The position (fig. 3) being the normal case investigated above, the case 4 was studied with 1,000 ohms in circuit, and for different lengths l of the quill tube c', beyond O, from $l = 2$ to over 40 cm. These results are given (fig. 5) the abscissas showing the length l of tube taken and the ordinates the fringe displacements s, both for the region R' alone (positive displacements here denoting dilatation) and for the region R (positive displacements denoting pressure). The results are periodic in marked degree, so that the quill tube (fig. 4) is a musical instrument with a pinhole embouchure; and in fact while the case 3 is nearly silent, 4 audibly reproduces the sound of the telephone. The curve for R' shows two reso-

nance maxima and one minimum, but in all cases the dilatation (positive-displacement s) within R' is sustained, merely changing in degree. The curve for R, however (dilatations for negative s) indicates the occurrence of both dilatations and pressures within this (smaller) region. Both curves are quite consistent (although R' is nearly four times more capacious than R) and one may infer the length of pipe $l = 30$ cm. (fig. 5) or the wave length $2l = 60$ cm. to be an harmonic of the telephone interruptor. This was in fact close to the four foot c.

In a majority of cases the action of the conical vent thus recalls the behavior of the cup anemometer, as the pressure excess is on the concave side; but the lower curve of figure 5 (for R smaller) is out of keeping with this, as between lengths of 20 and 35 cm. of pipe, the pressure excess is within, or on the salient side.

Resonators of Very Large Capacity.—The volume of the region R' (fig. 1) was now further increased by adding an additional cylindrical tube 6.2 cm. in diameter and 10.7 cm. high, closed on top with a glass plate. All parts were (as before) carefully cemented together. The volume added was thus 370 cm.^{3}, as compared with the original 48 cm.^{3}, the ratio of volume increment being 6.7, and the ratio of total to original volume 7.7 The conical vent c', figure 1, here acted much better than the copper foil pinholes c.

Using the motor break and conical valve, the fringe displacements were observed for frequencies between the notes g' and c''' and both with 1,000 ohms and 500 ohms in the telephone circuit. An example of the results is given in the two curves in figure 6, the region R being in communication with the atmosphere and R' closed except as to the salient conical vent specified. The curves are remarkable because of the sharpness of the maxima, which are apparently overtones in the key of B or B flat. It is obvious that the fundamentals of the large closed reservoir R' will lie very low as compared with the frequencies of the diagram and very large fringe displacements may be looked for and were found there. The height of the maxima gradually decreased from g to c''' there.

It was now thought desirable to test the conical vent in the reentrant position and data of this kind are given by the curve in figure 7. All the maxima are here dilatations, laid off positively for convenience in comparison with figure 6. The valve action (500 ohms in circuit) is much weaker in figure 7 than in figure 6; but in every other respect figure 6 is symmetrically reproduced. This is a very disconcerting result; for it is not the impulsive displacement of the telephone plate alone which produces the pressure increments within, even if a reversal of the telephonic current (change of poles) changes pressure increments into pressure decrements. The reversal conical pinhole vent in association with a given adjustment of telephone current (poles not changed) will do the same.

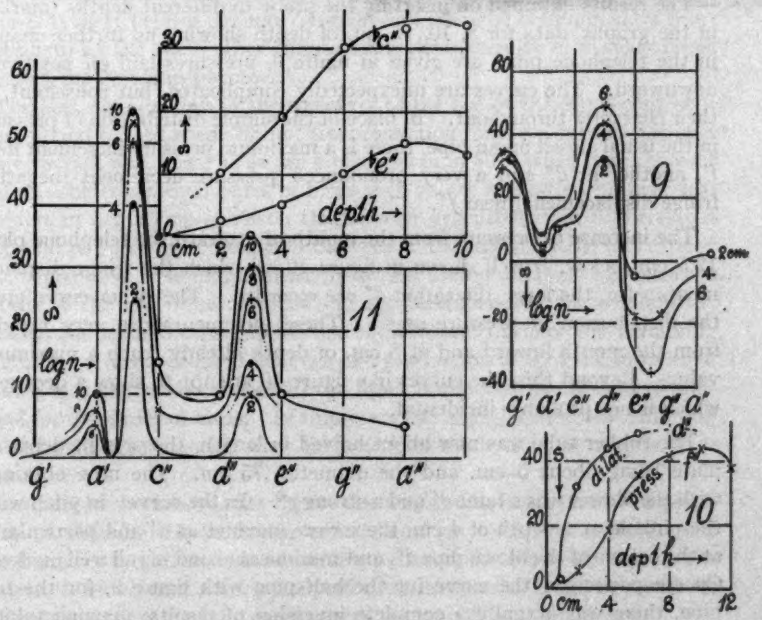
Resonator of Very Small Capacity.—Finally the resonator R' was all but

closed (resonator IV), or at least reduced to a shell-like space by a cylindrical inset, closed with a glass plate but a few millimeters above the mercury of the U-tube.

A survey of the fringe displacement corresponding to different harmonics is given in figure 8, 1,000 ohms completing the telephone circuit. Their distribution is less regular than heretofore, and probably the smaller serrations have escaped detection. What particularly astonishes is the occurrence of resonance at the low notes, seeing that the resonator volume is here relatively negligible. The maximum displacement of 100 fringes corresponds to a pressure increment of about .03 cm. of mercury, which though twice as large as obtained with the large resonator III, is nevertheless of the same order of values.

In one respect the present curve (fig. 8) for the shell resonator IV, differs radically from the complete curve for the capacious resonator III. In the latter, conformally with the large volume, the lower notes (8-foot octave) are very much more effective than the notes of the one foot octave. The curve as a whole falls from left to right. In figure 8 the reverse tendency is observed.

The question as to what resonates in case of the small resonator was approached by lengthening the tubing between the telephone and U-tube from its usual length of 48 cm., to 85 cm. and 120 cm. successively. The



shell-like volume of the resonator IV cannot much have exceeded 10 or 15 cm.^{3} The tubes added 10, 17, and 24 cm.^{3}, respectively. To this the shallow volume in the telephone (about 2 cm.^{3}) is to be added; nevertheless the greater part of the total volume seems to reside in the tubes.

The results obtained in the frequency survey for maxima and minima showed that the overtones had been differently accentuated. Thus the minimum near d'' passes from positive to negative values as the tube length increases, while the reverse is observed at the maximum near g''. The case for tube additions is then analogous to the occurrence of the vowel sounds, where different overtones are accentuated by variation of the mouth cavity.

The Pinhole Sonde, or Probe.—The possibilities of sounding for pressure by aid of a pinhole valve, O, at the end of a slender tube, c (as in figure 2), communicating with the closed reservoir R' of the U-tube, the other, R, being open to the atmosphere are next to be treated. The telephone T was at first provided with a tubular projection t, 12 cm. deep to the plate and about .75 cm. in inner diameter. This received the probe c (O being at the end of the aluminum tube C 2 mm. in bore, 3 mm. in outer diameter and about 20 cm. long) to different depths, with a view to exploring the pressure within.

The results obtained on inserting the probe to different depths (marked in the graphs, data for 8, 10, 12 cm. of depth showing no further change in the telephone pipe) are given in figure 9, pressures laid off positively downward. The curves are unexpectedly complicated, but consistent, in their character throughout. In place of the simple distribution of pressure in the usual closed organ pipe, there is a maximum pressure increment near f', another at d'' and a very pronounced pressure decrement (negative fringe displacement) near f''.

The increase of pressure from the mouth of t toward the telephone plate 12.5 cm. below, is well shown in figure 10, in which the fringe displacements s for the large dilatation d'' are recorded. The other curve gives the distribution of pressure at f''. These increments rise very rapidly from the mouth inward and at 8 cm. of depth already reach a maximum value. Beyond this, the curves like figure 10 are apt to show a decrease, which is not probably incidental.

The rubber tube was now about halved in length, the present depth to plate being about 5 cm. and the diameter .75 cm. The note obtained with the blower was a faint d' and a strong g''. In the survey in pitch with the pinhole at a depth of 4 cm. there were maxima at d' and particularly at the octave of the blown pipe d'', and minima at c' and a', all well marked. On comparison of the curve for the half pipe with figure 9, for the full pipe, there was actually a complete inversion of results, maxima taking

the place of minima. The largest displacement occurred at about 5 mm. from the plate.

Pressures in Smooth Straight Pipes.—These surveys in pitch and depth were completed for a number of telephone blown brass pipes, 1 cm. in diameter and of different lengths; but the very interesting graphs obtained must be omitted here. Thus the pipe 13 cm. long showed enormous maxima at a' and correspondingly large negative minima at d'', at all depths (2, 4, 8, 12 cm.) below the mouth of the tube. Figure 11 reproduces the behavior of the same tube cut down to 10 cm. of length, at different depths 2, 4, 6, 8, 10 cm. below the mouth. Finally similar graphs for a variety of wide tubes and resonators have been worked out and progress made with the installation for symmetrical induction.

THE PREDICTION OF ANNUAL EGG PRODUCTION FROM THE RECORDS OF LIMITED PERIODS

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For the past several years the writers have been considering the possibility of predicting the annual egg production of the domestic fowl from the records of short periods of time. Such records may be determined by trap-nesting, or by the use of other criteria when the maternity of the eggs is not required for breeding purposes.[1]

The first definite step in the direction of the use of the egg record of a short period for the prediction of the production during a subsequent or a longer period was, as far as we are aware, taken in 1917 when it was shown[2] that in a heterogeneous series of birds such as are submitted by practical breeders in egg laying contests, the October egg production is correlated with that of every other month of the year. The investigation was carried much further in a second memoir[3] in which the correlations between the records of the individual months and the production of the whole year, between the records of the individual months and those of the remaining 11 months of the year, and between the production of 5 of the individual months and the production of all the other individual months, were published for two series of birds. In this paper the equations for the prediction of total annual production from the record of the individual months were given.

Our purpose here is to state briefly the results of a first test of the possibility of utilizing the linear regression equation (which is strictly valid only for the population from which it is deduced) for the prediction of the records of the birds of a flock the performance of which is unknown as far as the determination of the constants of the equations is concerned.

In a population the straight line relating the egg production of a period E_p with that of a period used as a basis of prediction, e, is

$$E_p = \left(\bar{E}_p - {}'eE_p \frac{\sigma E_p}{\sigma_e}\ \bar{e}\right) + {}'eE_p \frac{\sigma E_p}{\sigma_e} e$$

where the bars denote means, the sigmas represent standard deviations, and r indicates the correlation of the two variables in the standard population.

The value of E_p given by the equation is the theoretical mean production for the array of individuals of any class with respect to e. The assumption to be tested is that we may write E_p' and e' instead of E_p and e, where E_p' is the theoretical mean production for a period p of the array of birds of any grade of production e in the period used as a basis of prediction in a series of birds which are not involved in the data upon which the equations were based, but to which the equations are to be applied for practical purposes.

The essential practical requisites for such prediction equations are: (1) That the errors of prediction shall be distributed about the true numbers in such a manner that estimation will not in the long run be either too high or too low. (2) That the magnitude of the deviation of the predicted from the observed egg production shall be as small as possible.

Let E_p'' be the actual and E_p' the predicted egg production of an individual bird for any period, p, in a flock to which the equation is being applied. The error of prediction is then $E_p' - E_p''$ The average of these errors, with regard to sign

$$\frac{1}{N}\Sigma\left(E_p' - E_p''\right)$$

furnishes a measure of the success with which the first requirement, (1) above, is met. The average of these errors without regard to sign furnishes a measure of the average error above or below the true production of the individual birds of a flock. The square root of mean square deviation

$$\left[\frac{1}{N}\Sigma\left(E_p' - E_p''\right)^2\right]^{1/2}$$

furnishes a measure of this error which weights larger errors.

The errors may be expressed in actual numbers of eggs, or, in relative terms, as percentages of the mean production of the period and flock for which prediction is made. Both methods have been used in testing the equations.

In testing the efficiency of such equations for purposes of prediction we have proceeded in a purely objective manner. Working on the assumption that the crucial test of any theory is its capacity for predicting

the unknown, we have calculated equations based upon the data of the International Egg Laying Contest at Storrs, Conn., during the six contest years, 1911–1917, inclusive. We have then used these equations to predict the annual production (and the production of groups of months) for the birds of the 1917–'18 contest, using as a basis of prediction the individual months of the laying year separately, pairs of successive months and groups of three months. Our conclusions concerning the value of the equations depend, therefore, not upon *a priori* considerations but upon the results of actual tests of accuracy of prediction for series which were unknown as far as the determination of the constants of the equations is concerned.

Consider first of all the results of the attempts to predict the annual egg production of 415 White Leghorn birds observed at Storrs from Nov. 1, 1917 to Oct. 31, 1918 from the records of a single month's production.

The results of the three criteria of accuracy of prediction are summarized in table 1.

TABLE 1

ERRORS OF PREDICTION OF ANNUAL EGG PRODUCTION FROM THE RECORDS OF INDIVIDUAL MONTHS

MONTH USED AS BASE OF PREDICTION	AVERAGE DEVIATION WITH REGARD TO SIGN		AVERAGE DEVIATION WITHOUT REGARD TO SIGN		SQUARE ROOT OF MEAN SQUARE DEVIATION	
	Actual deviation	Percentage deviation	Actual deviation	Percentage deviation	Actual deviation	Percentage deviation
November	+ 2.39	1.52	29.59	18.78	38.65	24.52
December	— 0.49	0.31	29.26	18.57	37.61	23.86
January	+ 2.58	1.64	30.09	19.09	38.77	24.60
February	+ 0.06	0.04	27.28	17.31	34.70	22.02
March	— 1.63	1.03	27.95	17.73	34.28	21.75
April	— 6.23	3.95	28.72	18.22	35.31	22.40
May	+ 7.02	4.45	28.62	18.16	35.89	22.77
June	— 5.21	3.31	29.03	18.42	36.53	23.18
July	— 5.27	3.34	28.35	17.99	35.89	22.77
August	— 0.82	0.52	26.87	17.05	34.34	21.79
September	+ 4.78	3.03	24.78	15.72	32.94	20.90
October	+ 3.95	2.51	27.37	17.37	36.47	23.14

Considering first of all the absolute values we note that the average errors with regard to sign are generally low. Thus the prediction from November and from January production gives on the average 3 eggs too many for the year. For December, February, March and August the prediction is in error by less than 2 eggs. The values predicted from April, May, June, July, September and October records are from 4 to 7 eggs in error.

The average deviations without regard to sign are of course much larger since they constitute a measure of the error of prediction of the records of individual birds. They range from 24.8 to 30.1 eggs. The significance of errors of this magnitude will be more clearly brought out later.

The square root of mean square deviation also shows considerable regu-

larity from month to month. These measures are naturally considerably larger than the average deviation without regard to sign. They range from 32.9 to 38.8 eggs.

It is clear that the annual egg production of birds similar in origin to the series upon which the prediction equations were based and maintained under similar conditions may be predicted with a relatively high degree of accuracy providing their record for any month is definitely known.

The order of the errors will be more readily understood by expressing them in relation to the average production of the flock, as shown by the percentage deviations.

We note that in predicting from December, February and August records the average error with regard to sign *is less than one per cent of the average annual yield of the flock.* In predicting from November, January and March the error lies between one and two per cent. When April, May, June, July, September and October records are used as a basis of prediction the average errors of prediction are from 2.50 to 4.50 per cent of the average annual yield.

The average deviations without regard to sign are less than 20 per cent of the annual production. The values for the individual months range from 15.7 for September to 19.1 for January.

The square root of mean square deviation is less than 25 per cent of the average annual production. The individual values range from 20.9 for September to 24.6 for January.

These two latter tests may at first seem to indicate very unsatisfactory prediction. Such is not, however, the case. These give the average errors *either above or below the true record made in the prediction of the results for an individual bird.* The thing which is required in practise is generally *the prediction for a group of birds of a particular grade* of egg record for the month used as a base of prediction. In a flock of 415 birds this has been shown to be possibe *with an error of less than 5 per cent of the annual production* when prediction is made from the record of *any* month of the year; and *with an error of less than 1 per cent when prediction is based upon the records of a number of the individual months.*

Lack of space precludes a discussion of the results of the prediction of the annual record of the bird from the combined record of two consecutive months. We may, however, illustrate the accuracy of prediction from the combined record of two consecutive months by means of the figures in diagram 1 which shows the accuracy of prediction from November plus December and from April plus May in comparison with the results of prediction from November and April. In these the estimated production is shown by a straight line.

The actual production for the year for which prediction is made is shown by solid dots for each group of birds as classified by monthly or bimonthly

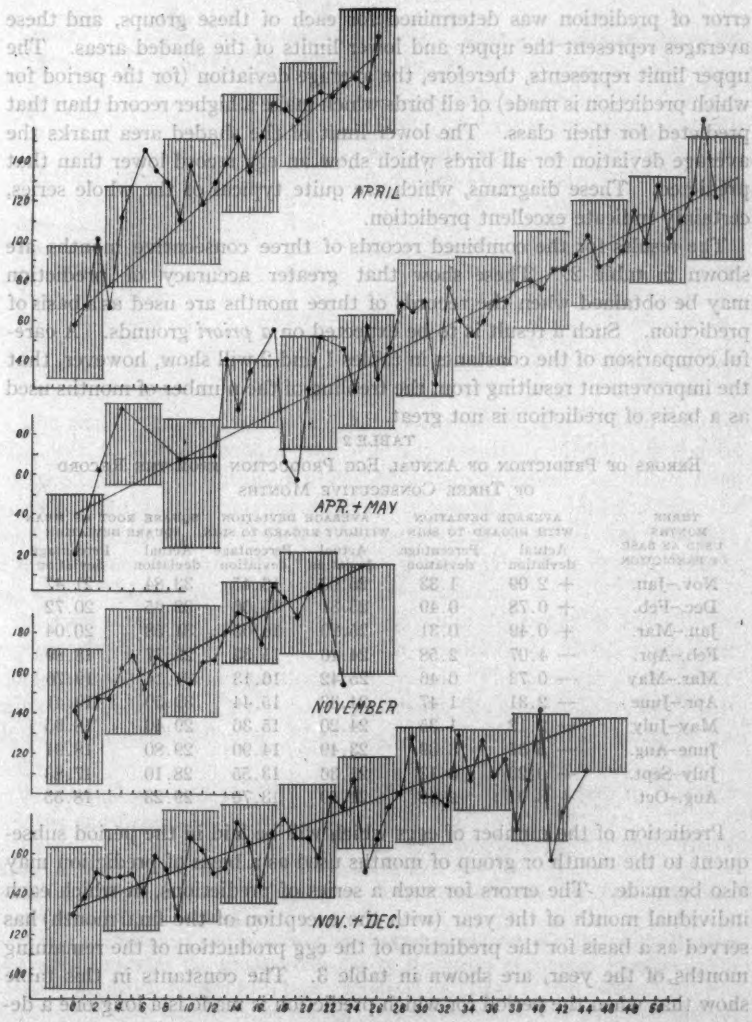

DIAGRAM 1

record. The shaded areas are determined as follows. The birds were first grouped into classes of five eggs range with respect to number of eggs laid during the period of time used as a basis of prediction. The birds of these classes of five eggs range were further subdivided into those in which actual egg production was greater than the predicted and those in which the actual number was less than the predicted number.[4] The average

error of prediction was determined for each of these groups, and these averages represent the upper and lower limits of the shaded areas. The upper limit represents, therefore, the average deviation (for the period for which prediction is made) of all birds which make a higher record than that predicted for their class. The lower limit of the shaded area marks the average deviation for all birds which show an egg record lower than that predicted. These diagrams, which are quite typical of the whole series, certainly indicate excellent prediction.

The results for the combined records of three consecutive months are shown in table 2. These show that greater accuracy of prediction may be obtained when the records of three months are used as a basis of prediction. Such a result is to be expected on *a priori* grounds. A careful comparison of the constants in tables 1 and 2 will show, however, that the improvement resulting from the trebling of the number of months used as a basis of prediction is not great.

TABLE 2

ERRORS OF PREDICTION OF ANNUAL EGG PRODUCTION FROM THE RECORD OF THREE CONSECUTIVE MONTHS

THREE MONTHS USED AS BASE OF PREDICTION	AVERAGE DEVIATION WITH REGARD TO SIGN		AVERAGE DEVIATION WITHOUT REGARD TO SIGN		SQUARE ROOT OF MEAN SQUARE DEVIATION	
	Actual deviation	Percentage deviation	Actual deviation	Percentage deviation	Actual deviation	Percentage deviation
Nov.–Jan.	+ 2.09	1.33	25.93	16.45	33.84	21.47
Dec.–Feb.	+ 0.78	0.49	25.31	16.06	32.65	20.72
Jan.–Mar.	+ 0.49	0.31	25.29	16.05	31.58	20.04
Feb.–Apr.	— 4.07	2.58	24.16	15.33	29.77	18.89
Mar.–May	— 0.73	0.46	25.42	16.13	31.14	19.76
Apr.–June	— 2.31	1.47	24.33	15.44	30.59	19.41
May–July	— 2.12	1.35	24.20	15.36	29.40	18.65
June–Aug.	— 5.35	3.39	23.49	14.90	29.80	18.91
July–Sept.	— 0.20	0.13	21.36	13.55	28.10	17.83
Aug.–Oct.	+ 3.91	2.48	21.59	13.70	29.23	18.55

Prediction of the number of eggs which will be laid in the period subsequent to the month or group of months used as a basis of prediction may also be made. The errors for such a series of predictions, in which each individual month of the year (with the exception of the final month) has served as a basis for the prediction of the egg production of the remaining months of the year, are shown in table 3. The constants in this table show that when the period for which prediction is made is a long one a degree of accuracy fairly comparable with that for the whole year is attainable. The absolute values of the average deviation without regard to sign and of the square root of mean square deviation necessarily become smaller as the period for which prediction is made becomes shorter. The relative (percentage) error, however, increases. Thus the accuracy of prediction decreases rapidly as the period for which prediction is made becomes shorter.

TABLE 3

ERRORS OF PREDICTION OF THE RECORD OF A PERIOD OF MONTHS FROM THE RECORD OF INDIVIDUAL PRECEDING MONTHS

PERIOD FOR WHICH PREDICTION IS MADE	MONTH USED AS BASE OF PREDICTION	AVERAGE DEVIATION WITH REGARD TO SIGN		AVERAGE DEVIATION WITHOUT REGARD TO SIGN		SQ. ROOT OF MEAN SQ. DEVIATION	
		Actual deviation	Percentage deviation	Actual deviation	Percentage deviation	Actual deviation	Percentage deviation
Dec.–Oct.	November	+ 2.39	1.57	29.59	19.49	38.65	25.46
Jan.–Oct.	December	+ 0.24	0.16	28.43	19.53	36.63	25.16
Feb.–Oct.	January	+ 2.37	1.71	26.48	19.06	34.61	24.91
Mar.–Oct.	February	+ 0.09	0.77	24.07	18.66	30.86	23.92
Apr.–Oct.	March	— 0.24	0.21	22.81	20.36	28.40	25.34
May–Oct.	April	— 4.00	4.23	21.36	22.59	26.50	28.02
June–Oct.	May	+ 3.62	4.98	19.89	27.35	24.81	34.11
July–Oct.	June	— 2.86	5.34	17.62	32.91	21.44	40.04
Aug.–Oct.	July	— 3.71	10.43	13.91	39.09	17.14	48.17
Sept.–Oct.	August	— 2.56	13.57	9.76	51.75	12.12	64.26
October	September	— 0.45	7.67	4.57	77.85	5.71	97.27

The results of this investigation, taken as a whole, show that in the case of a flock of White Leghorn fowl which is essentially identical in genetic composition and maintained under essentially uniform conditions from year to year it is quite possible to estimate annual egg production from the record of either a single month or of two or three consecutive months with a high degree of accuracy. The same is presumably true of other breeds as well. This point is now under investigation.

It is not possible to use the equations given in this paper for flocks differing greatly in genetic composition or in conditions of maintenance from that upon which these equations were based. The problem of the determination of corrective terms to be used when the equations are applied to flocks other than that upon which they are based is now under investigation.

A detailed account of these investigations is now in press in *Genetics*.

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[2]Harris, Blakeslee and Warner, These PROCEEDINGS 3, 1917 (337–341); Harris, Blakeslee, Warner and Kirkpatrick, *Genetics*, 2, 1917 (36–77).

[3]Harris, Blakeslee and Kirkpatrick, These PROCEEDINGS 3, 1917 (565–569); *Genetics*, 3, 1918 (27–72).

[4]A range of five eggs was used in order to obtain a number of birds sufficiently large to reduce somewhat the irregularities due to the errors of random sampling. The errors of prediction were in each case determined for classes of unit range. Grouping is used for graphic representation merely. The average deviations represented by the limit of the shaded zone are to be thought of as measured from a line perpendicular to the ordinates and intersecting the prediction line on the mid-ordinate of the 5-egg class.